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1945 Nos. 11 & 12

COLORADO BEETLE (*LEPTINOTARSA DECEMLINEATA* SAY) INFESTATION IN PIEDMONT, ITALY *

The liberation of North Italy having enabled renewal of communications with the Centre, news has been learnt of the presence of the Colorado beetle (*Leptinotarsa decemlineata* Say) in Piedmont.

Although not yet excessively serious, the infestation is in the Turin Province covering a stretch roughly 10 km. wide around Turin, and also in the provinces of Aosta, Cuneo and Alessandria, where, during the current year, smaller foci than in Turin Province have been observed.

The Royal Observatory for plant diseases at Turin has good reason to believe that this insect arrived in the autumn of 1943 in the first of the provinces indicated above, transported by German troops with potatoes from the Arc Valley, notoriously and heavily infested by the Colorado beetle from Lansleburg to Modane. During the 1944 summer, in effect, the presence of this pest was reported in the commune of Collegno, Turin Province, on an area a little over a hectare. The radical method of destruction could not be applied, the requisite flame-throwers not having been supplied, although they had been ordered in good time, and also because of the delay in making available the fuel and straw to be used for the same purpose in place of the flame-throwers.

Consequently, ordinary control measures were employed, such as direct collection of the insects and repeated spraying and dusting with arsenical preparations.

The winter treatment of the soil with carbon disulphide evidently did not give any result; infestation re-occurred in 1945 on a larger scale in the Province of Turin and in new centres, more limited, in the other provinces mentioned.

This being the case and in conformity with the provisions of the International Convention for plant protection, signed at Rome on April 16, 1929, and with the resolutions passed by the International Conference for the study of the control of the Colorado beetle held at Brussels on January 22 and 23, 1936, the Italian Government officially declares the presence of the Colorado beetle in the provinces of Turin, Aosta, Cuneo and Alessandria, and communicates that in the first-mentioned province the reporting of new centres of infestation and

* Communication from the Ministry of Agriculture and Forests, Rome, to the International Institute of Agriculture.

the execution of control operations (direct collection of the insects; destruction by fire of larvae and adults, arsenical treatments at intervals) have already been made compulsory by Prefectorial Ordinance No. 15491 of June 23, 1945; similar measures will be taken in the provinces recognized as attacked this year and in those which eventually will be so in the future.

FIRST OBSERVATIONS ON A MITE PARASITE OF THE PAPAW AND ON A PREDATOR OF SAME, IN ERITREA *

Since December, 1939-January, 1940, in the course of an investigational tour of the Tessenei Territory (western Altipiano Basso of Eritrea), I had the opportunity of observing for the first time extensive and severe attacks of a mite on all the papaw trees (*Carica papaya* L.) in an orchard garden of the S. I. A. situated on the southern bank of the Gasc.

Further observations on this parasite which I made during the first months of 1945 in the same region, but this time in the Cazzagon Concession situated in the district of Ghersai, Agordat, on the banks of the Barca, enabled me to follow very closely the development of this mite and of an active predator of same which I had never encountered before.

Later, that is, towards the end of June, other foci of this mite were found on papaws in the nursery of the Government Commissariat and in other plantations near Keren, and a few days later (beginning of July) further information and material for examination arrived from Ghersai.

In this note I will briefly expound the most outstanding points regarding the identity, range, biology, parasites and means of controlling the mite in question, postponing until later the publication of a more detailed contribution.

From the examination of the material sent to the eminent specialist E. A. MACGREGOR, at Whittier, California, on April 14, 1945, I recently learnt that the species in question is to be considered as identical with that called *Anychus latus* (C. et F.) by SAYED, mentioned in 1942 for Palestine and Egypt. The determination, however, cannot be held as absolutely definite owing to the description and vague figures of CANESTRINI which SAYED took as a basis to identify the material from Palestine and Egypt, and the schematic designs from the latter, and to the poor condition in which the specimens from Eritrea arrived to Mr MACGREGOR.

The geographical range of this mite in Eritrea, according to the investigations I made from 1939 to 1945, appears to cover all the western Altipiano Basso from an altitude of about 1,600 metres which is that of Keren, to approximately 530 metres which is the altitude of Tessenei. So far this species has not been observed in papaya groves which are placed up to about 1,900 m. at Udi Ugri and its environs, nor in the groves on the eastern slope to the meridian passing

* Communication from Prof. GIUSEPPE JANNONE, Chief of the Section of Entomology of the Department of Agriculture, Asmara, Eritrea.

by the Tabò Salomona Valley. To the east of this meridian, there are few papaws, as at Massaua, on which *A. latus* has not been observed. Its possibilities of existence, and consequently its range, appear to be closely connected with the climatic requirements of the species which has to have a fairly high temperature and a high degree of atmospheric moisture. These ideal conditions are to be found in the papaw groves situated on land cleared of *Hyphaene nodularia* Becc. stands along the banks of the Gasc, the Barca and some of their affluents and in other zones.

This species is polyphagous, attacking in an extraordinary manner the leaves, stems, flowers and the still green part of the trunk of both the male and female trees, and also the aerial parts of several other plants such as cotton, castor oil plant, different species of dwarf and runner beans and other plants of the wild herbaceous flora. When the larvae and adults pierce the young papaw fruits, these assume a characteristic aspect because of the small drops of latex which issue from the microscopic wounds, rapidly solidifying and remaining attached to the skin for a long time. The enormous number of mites which concentrate chiefly on the leaves, bring about in the dry season, from October to about April in the western Altipiano Basso, a profound crisis in many of the organs and especially in their function, thus causing total shedding of the flowers and the small fruit just set. Consequently, the damage ends in a check in production in the female plants for a period of several months.

The eggs of the mite, in the form of small subspherical, slightly flattened beads, mother of pearl colour when laid and deep orange after a few days because of the advance in the development of the embryo, are attached mainly to the under surface of the leaves, though also on the upper surface and on other parts of the plant. The parasite does not emit many silk filaments like the common red spider (*Tetranychus telarius* L) and others, and always lives uncovered on the leaves which become greyish-rust in colour, and also on the other parts of the plant already indicated.

A. latus, in the western Altipiano Basso of Eritrea, has several generations during the year, but its reproductive capacity diminishes perceptibly during the rainy season, perhaps because of the lower temperature. Its transport from one plant to another and from one plantation to one in the vicinity is effected directly or indirectly by means of wind, and animals and man onto which numerous individuals fall and adhere temporarily.

A fairly useful action in the field of biological control is exercized by a very small beetle (Coccinellidae), pertaining very probably to the species *Scymnus morelleti* Muls., already cited by WEISE for Keren where it was collected in the month of October, 1914, without indication of the host or trophic habits (cf.: WEISE, J. Chrysomeliden und Coccinelliden aus Erythraea. *Bollettino del Laboratorio di Zoologia generale e agraria della R. Scuola superiore d'Agricoltura in Portici*, Portici, 1914, vol. IX, pp. [227]-233). This determination, which I consider very probable, was effected by myself.

The predator in question as a larva and as adult feeds on *A. latus* in all stages of development. It lays its eggs on the leaves and on other aerial parts

of the plants visited ; the larvae are not very rapid in movement, but despite this easily get to all the parts infested by the mite ; the pupae are formed on the leaves and other parts of the plant and are firmly attached ; the adults, very agile, move about rapidly on their legs, but can also make short flights in search of plants loaded with their prey.

This *Scymnus*, however, does not succeed in checking attack by this mite from the economic standpoint, and therefore artificial control means have to be employed. The predator has several generations in a year ; it is also fairly common in the Ghersai and Keren zones.

During the hot dry season in 1944-45 in the Cazzagon Concession papaws and other plants were given repeated treatments of 1-1.5 per cent. solutions of concentrated tobacco extract with the addition of soap, and also 1 per cent. soapy kerosene emulsions. Because of the cost of the soap, other more economical adhesive substitutes were tried, such as very fine river silt, fairly good results being obtained. It was found that at least 3 thorough sprayings are necessary for a period of 3 to 4 months, also to be applied to other infested plants in the same zone.

The results of artificial control means can be observed immediately on the female papaw plants which again start flowering normally within a few days with a subsequent 100 per cent. fruit setting.

RESULTS OF THE STATISTICAL ANALYSIS OF SIX DISTRIBUTIONS OF GERMINATION PERCENTAGES OF CONIDIA OF *ALTERNARIA, TENUIS* TYPE *

In the statistical mathematical valuation of fungicides, the problem of the determination of the error in germination percentages obtained and the toxicity curve remained in abeyance. This problem was clearly put by the experimental data which deviated from the theory much more seriously and frequently than would be expected with the calculation of probabilities for simple accidental factors. This fact did not enable the direct application of ordinary methods of determining error (based on the BERNOULLI formula or on its inversion with the PRIGGE formula) ; therefore, the method of correction of WILCOXON and MCCALLAN, based on the importance of the deviations of each point of the line, was temporarily accepted.

In order to solve this question definitely, it was necessary to analyze the frequency distributions of non-germination percentages ; this was possible thanks to the existence of a large number of data useful for this investigation.

The results of these analyses, which synthesize the observations made on approximately 290,000 conidia of *Alternaria, tenuis* type, are particularly inter-

* Communication from Messrs L. CAVALLI and R. CIFERRI, Centre for the study of fungicides at the Italian Cryptogamic Laboratory attached to the University, Pavia, Italy.

esting as they reveal the existence of various systematic factors of error of biological and practical value. This analysis, however, cannot be considered as complete since, for the general solution of the problem, certain data, which will be indicated later, are wanting.

The distributions on which the analysis is based are the following:

TABLE I.

Non-germination in water:
distribution of N = 125 percentages on n = 100 conidia
» N = 232 » » n = 300 »
» N = 131 » » n = 500 »

Non-germination in Cu: Caffaro powder with about 16 per cent. Cu:
distribution of N = 168 percentages on n = 100 conidia
» N = 156 » » n = 300 »

Bordeaux mixture containing about 24 per cent. Cu:
distribution of N = 155 percentages on n = 500 conidia.

1. — Distributions of non-germination percentages in water.

These distributions, which are only of secondary importance for our purposes, gave statistically complex results and did not allow of an immediate biological interpretation. In the graphic analysis, the three curves proved plurimodal; in the first and third curve, three distinct modes are evident, in the second (n = 300) only two. The position of the modes is similar for all the curves; the first falls round about 3 per cent., the second 8 per cent. and the third (which is not seen in the second curve) about 14 per cent. A more exact determination of these modes and an analytical elaboration of the theoretical curves would be senseless for our purposes.

Two considerations on the standard deviation (σ) of the three distributions, the value of which is indicated in Table II, show that the plurimodality of the curves (and consequently the intervention of fixed systematic factors of variations of certain values of basic probability, close to those of the modes in question) is not due to the exiguity of the sample:

TABLE II.

1st n = 100	4.8 per cent.	2.8 per cent.
2nd n = 300	3.5 » »	1.5 » »
3rd n = 500	3.9 » »	1.2 » »

(1) The hypernormality of the distribution is attested by the LEXIS coefficient of dispersion (which, as is known, is given by the ratio between σ actually observed [first column] and σ calculated according to BERNOULLI [second co-

lumn], which assumes, for the three distributions, the following values: 1.71; 2.35; 3.25, namely, values assuredly higher than the unit beyond the limits of error).

(2) The relative invariability of the standard deviation, even with the increase in number of spores observed n, shows that with the increase of n, the values do not tend to regroup around a sole mode as ought to be expected and as is shown by the σ calculated according to BERNOULLI; but they maintained fixed round the several modes and consequently the zone of variability of which σ is the indirect measure, cannot be limited appreciably with increase in n.

In conclusion, the percentage of non-germination in water always indicates positive values, distributed in such a way as to give cause for suspecting the existence of systematic factors of variation which do not occur uniformly in all tests. The constancy with which in the three distributions all the values are grouped about the three fixed modes makes one think that it is not a question of factors, even systematic, of technique (among which the only important one would be the temperature to which the slides were exposed and which varied slightly from one experiment to another), but of true biological factors.

Among these latter factors, it is easy to determine one which could tentatively explain the phenomenon of multiple modes: the saltations in the culture of which the high frequency for Hyphomycetes of the same type is known. The plurimodality would thus be explained as being due to the fact that in some sub-cultures utilized in the experiments there occurred by chance one type, in others another type, while in still others an intermediate or mixed type predominated. This hypothesis which may explain the plurimodality of the curves (under certain conditions on which we shall not dwell) are not, however, of any great importance for the pursuit of this work; for this reason we did not deem experimental demonstration necessary.

The hypothesis just indicated explains the plurimodality of the curve, but does not yet adequately explain why, in the water control tests, there is frequently an appreciable non-germination percentage. The reason for this, which is still not ascertained, is important for the remainder of the formulation.

2. — Distributions of non-germination percentages in copper salt solutions.

First we will consider the two distributions for n = 100 and n = 300, because that with n = 500, owing to the special conditions under which it was obtained, gives different results.

The examination of the graphs (which are not reproduced here) and a comparison between the theoretical σ according to BERNOULLI and σ observed from all the data (Table III) leads to the conclusion that, at least for the two first distributions, we come up against a case different from that considered first: none of the three distributions presents secondary modes, namely, one is led to think that there is only one basic probability, and at least for the first two

distributions, the error observed is only slightly higher than that calculated in the case of a BERNOULLI distribution :

TABLE III.

Distribution	Average percentage	Observed σ	Calculated σ
n = 100	96.4	2.36%	1.90%
n = 300	96.9	1.51%	1.00%
n = 500	96.6	1.85%	0.83%

Consequently, it is worth while comparing these distributions with those which would be expected in theory if the plan of BERNOULLI was realized. The conditions under which this plan is valid in application to the case in question may, as is known, be summed up thus : it is permissible to consider the distribution of non-germination percentages as a BERNOULLI distribution, if it can be admitted that in each of the tests the basic probability does not vary, namely, that if non-germination (which is expressed empirically with the average of all the percentages, as the most probable value). Consequently, the fungus stock has to react in an identical manner at each determination, and since the different determinations were made within a period of some months, it is necessary that throughout this time the fungus does not vary in its susceptibility to copper. It is also necessary that the technique followed in examination is such that it cannot cause appreciable variations, even of an accidental nature. The gross increase in standard deviations observed according to the first data was rightly attributed to difficulties of a technical order. Since, even with the present more extensive material, there was an evident excess in observed σ compared with the theoretical, it could be thought that it is really due to a slight accidental variation in the basic probability, or to a Gaussian curve of accidental errors of measurement. In these two cases we will have the superposition of a BERNOULLI distribution and a distribution of a Gaussian type around the same average. The variations of these two curves would add up to give the total variation.

There is a source of error in the method of microscope reading by which a spore counted can no longer be reckoned (which would correspond to an extraction system in which the ball is not returned to the ballot-box after extraction and counting) and which thus causes deviation with the true BERNOULLI distribution, which is with replacing of the ball in the box. The correction factor which should be introduced here (and which has already been considered on other occasions by SCHAEFER, 1935) is, however, insignificant.

In adapting the distributions of BERNOULLI to the empiric distributions, however, a reflexion is immediately arrived at : the use of empirical averages (indicated in the table) leads to an adaptation which is not entirely satisfactory, while, on the contrary, a better adaptation is obtained by employing, as basic probability, the distribution modes (97 per cent. for n = 100, 98 per cent. for n = 300). This implied that the reason for the increase in empirical standard

deviation in relation to the theoretical one should not be attributed to accidental factors (which would have complied with the principle of arithmetical average as the most probable value for the distribution) and that consequently neither of the two accidental factors noted above can be concerned, but that the displacement of the average to the left is to be attributed to systematic deviations in the same sense.

Examination of the graphs shows, in effect, the existence of divergences to the left of such an extent (up to more than eight times σ) that they cannot be judged as accidental by any theoretical distribution. Moreover, percentages were obtained (not indicated here) even below 80 per cent., and which, therefore, cannot evidently be considered as being of accidental origin.

It is to be considered, therefore, that the deviations to the left (that is, in the sense of a greater resistance to copper) intervene with a certain frequency with an intermittent character and with a sudden modification of the basic probability which drops to very low values. The hypothesis is biologically acceptable as it is known that, with a not unimportant frequency, jumps are also produced spontaneously with changes of a biochemical character, including the resistance to copper. It is sufficient for such a jump to occur in one of the subculture employed daily for the determination of non-germination percentages, to alter appreciably the corresponding percentages. Jumps of this type would be produced in 2 per cent. of the experiments for $n = 100$ and 9 per cent. for $n = 300$.

The hypothesis can be checked experimentally, which, however, is of no importance for the purposes we have in view since the extent itself of the deviations proves that they are of a systematic nature and can, if necessary, be ignored in examination of the distribution.

As criterion of elimination of sistematic divergences, we have used the common criterion of eliminating the values which fall over three times the deviation standard calculated from an average corresponding to that of the most suitable theoretical distribution.

Neither with this criterion nor with others, however, was it possible to eliminate the systematic divergences which fall within the three σ and which cause a slight asymmetry of the curve on the right. They are not important, however, and only alter the results of the x^2 criterion for examination of the goodness of fit, owing to the error they cause in the left marginal frequencies (especially important in the distribution for $n = 300$, which has a higher percentage of deviations, beyond the 3 σ).

As the average values of the empiric distribution cannot be utilized for calculation of the basic probability p, it was possible to come to an adjustment by seeking by experiment the basic probability which gave the best adaptation to empiric distributions. The corresponding formulae are:

$$\text{for } n = 100, \text{ expansion of } 163\ (0.03 + 0.97)^{100};\ p = \frac{(97)}{100}$$

$$\text{for } n = 300, \quad \text{»} \quad \text{»} \quad 143\ (0.023 + 0.977)^{300};\ p = \frac{(293)}{300}$$

For the distribution $n = 300$, the best adaptation is obtained by making use of a basic probability $p = \frac{293}{300} = 0.977$, fairly close to the average of the empiric percentages with exclusion of data under -3σ. In no istance, however, does χ^2 go beyond the minimum limit of probability.

For the distribution $n = 100$, a better adaptation is obtained with the use of the basic probability $p = 0.967$, which is the average of the observations excluding those of the values beyond the -3σ (namely, the values below 91 per cent.). But the χ^2 of this which is the best adjustment is still unacceptable (it corresponds to 5 degrees of freedom and $p = 0.02$. It may be seen that there are different systematic factors which we will shortly examine and which influence the curve and which perhaps make it impossible to obtain a satisfactory adjustment with χ^2.

(1) We have already specified the first of these factors in the presence of systematic deviations to the left within the limits of 3σ for which there is no practical elimination criterion. This is especially evident only in the distribution $n = 300$, in which also the deviations beyond 3σ are most frequent.

(2) The fact that the basic probability p may be considered as constant only in the first approximation. For example, it varied very significantly during the time which elapsed between the experiments on $n = 100$ and on $n = 300$. The difference between the two averages, taken in Table II, is approximately 2.2 times the standard deviation, and this ratio is still greater (about 3.5) if account is taken of the corrected averages, that is, the same probabilities seen above *.

(3) The distribution of percentages of non-germination in water shows that part of the spores (whith two principal modes at 4 and 8 per cent.) do not germinate in the control tests. If it is admitted (and we will examine later the conditions of experimental research) that the spores which do not germinate in water are to be considered as dead and that the toxic action of Cu is produced only on the remaining spores, the distribution of the non-germination percentage in Cu will have to be corrected, value for value, by that of non-germination in water. Also the distribution of percentages of non-germination in Cu will consequently be plurimodal, but this plurimodality will not be practically expressed since, in allowing that the probability of true action to be taken as 97 per cent., the two modes will be 97 and 97.3 per cent. Consequently, the error, although slight, exists and will be much more evident with weak solutions of Cu, which will cause a lower percentage of non-germination: the more so the smaller this percentage.

(4) The following is a systematic factor of error to the left. It sometimes happens that in counting the spores (done semi-automatically), the anticipated total is exceeded (*e. g.*, $n = 100$). In this case, the operator evidently deducts units (back to 100) only from the higher of the two figures obtained (non-germinated spores), and thus the percentage of non-sporification is under-estimated.

This occurrence naturally, because of its rarity and small extent (there are never more than 3 or 4 conidia over 100) does not show in the distribution curve, especially

* There is, however, a possibility of error in this hypothesis ; see point 4.

in that for $n = 300$. It may, perhaps, explain the difference in average between $n = 100$ and $n = 300$. At all events, since not much importance was attributed to this significant difference, we will not go into it further.

General considerations on biological distributions, which very rarely fully satisfy conditions of validity of theoretical diagrams, and on the inevitable errors in all statistical data, some of which have been dealt with here *, lead us to consider as satisfactory for practical purposes the adjustment here obtained for a BERNOULLI distribution for the distributions of percentages of non-germination of conidia subjected to the action of a fungicide, when there is the possibility of eliminating systematic deviations over the three σ. When only one observation (or only a few) is available, this is evidently impossible ; but when a toxicity curve has been drawn, it is easy to see if certain points deviate beyond 3σ (which corresponds to the fact that the curve does not cut an interval for $K = 3$) and to eliminate them from the distribution, which is justified by the considerations made.

The nature of the distributions, all around an extreme value, does not allow of observing if the systematic deviations are produced not only to the left but also to the right, which appears biologically possible. Also without resorting to a distribution for the intermediary percentages (around 50 per cent.), a fairly exact idea may be given by the examination of a sufficient number of toxicity curves. In effect, the deviations to the left in the distribution will fall below the toxicity curve and those to the right above.

Attention has not been given to the case of $n = 500$, which was obtained with a different fungicide, at a different strength, but chiefly on three different clones of funghi. For this reason, examination of the corresponding graph is very instructive. It shows a much greater dispersion than the theoretical one according to BERNOULLI, which moreover could already be seen from the appreciable difference between measured σ and calculated σ in Table III. It may be concluded from experience that, from clone to clone, the basic probability varies significantly, even if it is a question of few percentage units. The use of the same clone for comparable experiments between them, therefore, is indispensable.

In conclusion, the use of the PRIGGE or BERNOULLI formula is permissible when the values are deviated beyond the three σ of the theoretical value, by utilizing the toxicity curve as line of theoretical values, as already stated above. Later we will see another possible limitation of this use.

3. — Discussion of results and conclusions.

Different hypotheses have been put forward and several problems are not solved :

(*A*) Causes of partial non-germination of conidia in water ;

(*B*) Their relation with non-germination in fungicides ;

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* The analysis of the variation effected by one of us (CIFERRI) and by BARBENSI revealed the extension of various other errors in technique, leading to the conclusion that none of them gives rise to significant differences when work is carried out under suitably controlled and standardized conditions.

(*C*) Causes of the plurimodality of the distribution in water (reality or otherwise of the 'jump' hypothesis) and its influence on the distribution of percentage of non-germination in copper ;

(*D*) Causes of systematic deviations observed in percentages of non-germination in fungicides (hypotheses of Cu-resistant mutations).

Since only the first two problems are of immediate and practical importance, it is these which we will briefly discuss.

(*A*) Ascertaining of the causes of non-germination of conidia in water.

The possible causes are essentially :

(1) Toxic action of the water employed (traces of heavy metals in distilled water ?).

(2) Non-germinability of the conidia in the cultures.

(3) Slow rate of germination, numerous conidia had not yet germinated at the time of observation.

The distinction between the first, second and third cause may be made simply by checking the rate of non-germination percentages, that is, by reading the same slide after, for example, 12, 24, 36, 48, 60 hours, etc. If the percentages of non-germination diminish in time, the third cause will come into play ; if they remain stationary, the second ; if they increase, the first and also the second. The investigation would have this importance : if the 2nd cause is in play (and perhaps also the 3rd), the conidia already dead cannot germinate in the copper salts either, and if the germination percentage obtained in copper is not corrected, this will be systematically over-estimated. If the 2nd cause is true, there should however, also be a positive correlation between the percentages determined contemporaneously of non-germination in water and in copper.

(*B*) Investigations on the correlation between percentages of non-germination in water and in copper.

If this correlation exists and is positive, this means that one of the causes *A*, 2 or *A*, 3 will come into play. Its existence, therefore, will corroborate the data of these investigations ; but it is sufficient, in itself, to require a correction of empirical percentages of non-germination in copper by control percentages in water, even for the percentages utilized in the toxicity curve.

Whence the importance of this investigation.

The correction to be applied in the case where conidia not germinated in water would be dead (or equivalent), is the following :

With x' as the percentage of gross non-germination in Cu, and a the percentage of non-germination in water (dead conidia), x the unknown toxicity percentage of the fungicide on the live conidia ($100 - a$), we have :

$$x' = x\,(100 - a) + a$$

whence :

$$x = \frac{x' - a}{100 - a} \qquad (1)$$

Naturally x thus determined no longer contains only the error of x', but also that of a (both computable with the BERNOULLI formula).

In a first approximation, and only taking into account the numerator of (1), if $s_{x'}$ is the standard deviation of x', and s_a that of a, we will have :

$$s_x = \sqrt{s^2 x' + s^2 a} \qquad (2)$$

with the usual formula.

THIRD CONTRIBUTION TO THE LOCUST BIBLIOGRAPHY *

concluded †

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* Drawn up by GIULIO TRINCHIERI, International Institute of Agriculture.

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